fertility depended on such delicate adjustment of the male and female elements to each other that, unless constantly kept up by the preservation of the most fertile individuals, sterility is always ready to arise. . . . So long as a species remains undivided, and in occupation of a continuous area, its fertility is kept up by natural selection; but the moment it becomes separated, either by geographical or selective isolation or by diversity of station or of habits, then, while each portion must be kept fertile *inter se*, there is nothing to prevent infertility arising between the two separated portions" (p. 184). Here is an application of the principle of segregation, or of like to like in groups that do not cross, in which indiscriminate separation is followed by increasing divergence in the different portions, not because they are exposed to different environments, not because there is any advantage in such divergence, not because there is any need that the function should be performed more perfectly in one portion than in the other, but because intergeneration, which is the principle by which correspondence of function is secured, has been suspended for some generations; and, in the absence of intergeneration, neither natural selection nor any other principle is capable of preserving complete correspondence. In organisms that reproduce sexually, the causes of divergence are many, though they may all be classed as causes of segregation, while the causes of correspondence with variation, whether of functions or of structures, are causes of intergeneration between partial segregations. If the environments surrounding the isolated portions are the same, the *use* of the environment, and therefore the forms of natural selection, may become divergent; if the use continues unchanged, some *useless divergence in the method of securing the use* may appear. Or, if all the relations to the environment, whether useful or useless, remain unchanged, "the adjustment of the male and female elements to each other" are liable to become slightly divergent, producing *mutual infertility*. Or the *preference of the sexes* for certain shades or arrangements of colour in their mates may become slightly different. Through some slight difference in the hereditary elements, distributed in each separated portion at the first, one or all of these causes of accumulated divergence may be introduced. I think it is evident that we have here a general principle, which is as applicable to a wide range of divergences as it is to the divergence that produces mutual infertility and sterility.

The context shows that the prominent idea in Mr. Wallace's mind was divergence in the adjustment of the male and female elements through correlation with "some diversity of form or colour," resulting from divergent forms of natural selection, that had been induced by exposure to "somewhat different conditions of life." But if the reasoning is correct in the sentences I have quoted, it gives an explanation of similar divergences when the separated portions are exposed to the same environment, *and where there is no possible advantage to be gained by divergence*. This is one of the principles I have used in the explanation of the divergences of Sandwich Island land mollusks; and I think that in the earlier stages of the development of infertility between allied forms it is often the only explanation that is applicable. It should, however, be remembered that, for divergence of this kind, it is not always necessary that the isolation should be either complete or very long continued, and that, when the forms that are not fully fertile with each other meet and more or less commingle, there is, through the very laws of propagation, without any aid from natural selection, a constant increase in the ratio of the pure breeds to the mongrels, and an accumulating intensity in the segregative instincts and the physiological incompatibilities. As this point has been fully discussed in my paper on "Divergent Evolution," I do not need to enlarge on it here (see Linn. Soc. Journ., Zoology, vol. xx. pp. 246–72).

There is, however, another phase of the subject which is indicated by Mr. Wallace's suggestion that infertility depends on "such a delicate adjustment" that it is more easily affected by isolation than some other adjustments. This is, I think, a very interesting point, as it suggests how it is that, in some cases at least, physiological divergence of this kind *is one of the first forms of divergence that arises*. But in some species other adjustments seem to be more delicate than this, and therefore more easily disturbed; while in others, several sets of adjustments, as colours and other recognition marks, with the preferences that correspond, and the habits of feeding and defence are in a state of equilibrium, the stability or instability of which is about the same as of that which determines the relations of the male and female elements. In this last class of cases, several forms of divergence may arise during the same stage of development, and that too when the isolated portions are exposed to the same environment. In some species a large number of characters are in a state of unstable adjustment. As Prof. Lankester has suggested near the close of his review of Mr. Wallace's book, this cause of divergence seems to be specially operative in the case of human faculties. But variability with plasticity of type is not the only condition that affects the stability of segregated portions of a species. Other things being equal, a single pair of any species is much less likely to represent the average of all the characters of the species than a million pairs. This consideration throws light on the comparative lack of divergence between the land animals of England and those of Ireland, which lack has been referred to by Mr. Wallace as an objection to my theory. In this case, many millions of some of the species were probably existing in each district at the time of the separation. As Prof. Lankester has pointed out, the representatives of the human species in the two districts have somewhat diverged; and the probability is, that if we were equally acquainted with the other species, we should find other examples of divergence in minor points. If the isolation is made more complete, and is longer continued, I believe the divergence will gradually become more apparent.

Mr. Wallace has mentioned another class of divergences which are best explained by the principle we are now considering—as he seems to have apprehended, though the process is not stated here as clearly as when discussing the divergences that produce infertility. The passage is as follows:—"The enormously lengthened plumes of the bird of paradise and of the peacock must be rather injurious than beneficial in the birds' ordinary life. The fact that they have been developed to so great an extent in a few species is an indication of such perfect adaptation to the conditions of existence, such complete success in the battle of life, that there is, in the adult male at all events, *a surplus of strength, vitality, and growth-power which is able to expend itself in this way without injury*. That such is the case is shown by the great abundance of most of the species which possess *these wonderful superfluities of plumage*. . . . Why, in allied species, the development of accessory plumes has taken different forms, we are unable to say, except that it may be due to that individual variability, which has served as the starting-point for so much that seems to us strange in form, or fantastic in colour, both in the animal and vegetable world" ("Darwinism," p. 293. The italics are mine).

It is no small gratification to me that Mr. Wallace has found this principle of unstable adjustment worthy of application to two important classes of divergences; and that, in the case of one of these classes, he has recognized that correspondence in such adjustments cannot be continuously maintained between the isolated portions of a species. I, moreover, have some hope that, when he understands the relation in which instability and isolation stand to each other in my theory, he will admit that it throws some light on the remarkable divergences of Sandwich Island land mollusks. The subject was only incidentally touched upon in my paper on "Divergent Evolution through Cumulative Segregation," but will be more fully discussed in a supplemental paper on "Intensive Segregation."

26 Concession, Osaka, Japan. JOHN T. GULICK.

Coral Reefs, Fossil and Recent.

MANY Alpine geologists believe the limestone and dolomite mountains which form so peculiarly beautiful and interesting a part of our Eastern Alps to be in great part composed of Triassic coral reefs. If this be so, their geological structure must necessarily contribute much towards elucidating the discussion concerning the origin of atolls and other forms of recent coral reefs. In this discussion, which has chiefly been carried on in England, the structure of our Triassic limestone mountains has been left out of account in a manner very surprising to me, considering that authorities like Richthofen and Mojsisovics have declared them to be remnants of coral reefs.

I have made a number of Alpine ascents in the dolomites of South Tyrol, chiefly in the districts of the Höhlensteinthal, Primiero, and the Langkofel, and have satisfied myself that the theory of the coralligenous origin of great part of these mountains is the only one which will explain the position and nature of the rocks composing them.

Not only do we observe in many places the massy dolomite alternating at its margin with sedimentary deep-sea deposit of partly non-calcareous nature, but we even find old reef surfaces exposed to view. The volcanic porphyritic lava, or rather tufa, which was spread over the sea-bottom after the termination of the Buchensteiner period (middle Triassic, Mediterranean province) covers the deep-sea deposits of earlier date, but leaves the masses of dolomite free. Here and there, as on the *Plattkofel*, it can be seen overlying the foot of the actual reef precipice and there ending. It is covered by similar dolomite of a later date.

Many observations by Dana, Walther ("Korallriffe des Rothen Meeres"), and others, have shown that old coral reefs, about the nature of which there can be no doubt, are often dolomitic. The structure of Tertiary coral reefs on the Sinai peninsula, about the origin of which no doubt can be entertained, is actually identical with the structure of some Triassic dolomite I have examined here. I believe myself, for these, and many other reasons, justified in agreeing with Richthofen, &c., and in assuming that many Tyrolese Triassic limestones and dolomites are coralligenous.

The zones of the Mediterranean Trias differ altogether from the Trias in Germany. Other limits must here be recognized. In this respect I follow Mojsisovics. In the lowest Triassic no coral reefs are observed; also in the zones corresponding to the German Muschelkalk, we find, although these deposits are usually calcareous, no reefs of any size. It is in the zones distinguished as Buchensteiner, Wengener, and Cassianer-Schichten, that the great massy, unstratified reefs of South Tyrol are met with. The Upper Triassic layers, known as Raiblerschichten and Dachsteinkalk, are in South Tyrol mostly stratified, and in my opinion sedimentary, not coralligenous. Numerous faults traverse South Tyrol and break up the whole, only slightly folded Triassic system, into numerous plates (*Tafeln*) which are elevated on one (usually the northern) and depressed on the other (usually the southern) side. Liassic, Jurassic, and even Cretaceous layers rest on the depressed margins of the plates. Elsewhere these have been entirely removed, and the underlying Triassic reefs, capped with remains of sedimentary Dachsteinkalk and Raiblerschichten, have been laid bare. On the elevated margins along the faults also the Triassic layers have been removed. It is clear that somewhere between the subsided margin of the plates covered with Jurassic deposit, and the elevated margin, laid bare down to the Dyas, there must be places where the erosion has just reached the middle Triassic reefs. Here it is that we find parts of natural reef surfaces exposed.

The Lower Triassic Werfener Schichten are divided from the middle Triassic by deposits of gypsum, which show that the sea receded after the Werfener Schichten had been formed. Afterwards the sea returned, and it is clear that it must have risen at least as high as the later layers are thick, whilst or before they were deposited. The fossils in the sedimentary deposits between the masses of structureless dolomite show that the depth increased during their deposition. These sedimentary deposits alternate, as above stated, at their margins with the dolomite at the foot of the reef precipices. Therefore the dolomite also was formed during the rise of the water, for which I shall henceforth use the more exact expression introduced by Suess—positive shifting of the coast-line.

The dolomite masses are coral reefs. They have been formed during a period of positive shifting of coast-line, therefore we may assume that the high coral reefs now living and growing may have attained their astonishing altitude over the sea-bottom during a period of positive coast-line shifting. The dolomite masses of the Wengener and Cassianer period show no trace of stratification, such as is observed in the lower Muschelkalk and in the higher Dachsteinkalk. Moreover the dolomite has the same perfectly uniform structure from top to bottom; and on the vertical cliffs produced by erosion, which are often several thousand feet high, no trace of a stratified basal part can be detected. Everywhere the massy dolomite rests on discordant older layers, or—as usually at the reef margin—on the simultaneously deposited deep-sea sediment.

I will now proceed to utilize the facts here outlined in criticizing the discussion between the advocates of Darwin's and Dr. Murray's theories concerning the origin of coral reefs. I may say at once that all the phenomena observed in the dolomites of South Tyrol corroborate Darwin's subsidence theory, whereas they do not find explanation if we accept Dr. Murray's. It is the latter, therefore, which requires a closer examination.

Dr. Murray says that on slight elevations of the sea-bottom calcareous sediment accumulates, whereas in the greater surrounding depths this is not the case in consequence of the increase of dissolving power of sea-water with increasing depth. I accept this, and I believe that the caps of stratified Dachsteinkalk on the summits of many of the middle Triassic reefs in South Tyrol have been formed in this manner after the growth of the reefs had terminated. The positive coast-line shifting led to a horizontal extension of the Triassic Mediterranean northward, and a junction with colder northern seas, which caused a cooling of the water in the bay of South Tyrol, and thereby terminated the existence of reef-building corals. The positive coast-line shifting continued, and during its progress the Dachsteinkalk was deposited on the summits of the reefs, whilst the intermediate deeper spaces were left free from calcareous deposit—in accordance with Dr. Murray's view. There is, however, as above stated, nowhere a trace of stratified calcareous sediment forming a *basement* or nucleus to any one of the dolomite masses.

Dr. Murray then goes on to say that, as soon as the accumulating calcareous sediment has reached the region of coral growth, say the 20-fathom line, corals will grow on it, and an isolated atoll rising precipitously, perhaps ten thousand feet from the sea-bottom, will be formed. Against this it must be objected that the soft Globigerina ooze, which forms the whole of the atoll-peak, with the exception of an insignificant coral cap, could never attain such precipitous slopes as the atolls usually have. A slope of 45° or more could never be formed. The fossil deposits of this kind observed in South Tyrol (*Uebergussschichten* of Mojsisovics) nowhere terminate abruptly like the reefs. Neither is a slope of this kind anywhere observed in the region of the Dachsteinkalk.

Then, according to Dr. Murray, an atoll is formed by the solution of the lime in the centre of the reef. Although the Oolithes discovered in reef regions by Walther show clearly enough that there cannot be any solution other than what is compensated by redeposition, in any enclosed lagoon, I would like to draw attention to the logical discrepancy in this part of Dr. Murray's theory. First, a limestone cone is built, because the lime is deposited more rapidly than it can be dissolved. Then a lagoon is formed because the solution exceeds the accumulation, and this on the same spot, in still shallower and less powerfully dissolving water, and in spite of the relative stagnancy of the water in the lagoon and the limestone material, which is continually washed into the lagoon from the parts of the surrounding reef, which lie above the level of the sea. I think that gives the *coup de grâce* to Dr. Murray's atoll theory.

There remains yet something to be said on his ideas concerning the lateral growth of reefs. There can be no doubt that there is such lateral growth, and that the band of living coral on the reef margin can advance towards deeper water on a basis of coral fragments which have fallen from the outward growing face of the reef. As the corals near the surface grow more rapidly than those further down, the advancing reef face must ever tend to become overhanging; parts of the living coral must therefore frequently break off, fall down, and accumulate below. But there is a limit to this lateral growth which restricts it so considerably that it will by no means explain the formation of masses of dolomite 4000 feet thick, like the Cimon della Pala, for instance; and far less will it enable an atoll rising 10,000 feet or more from the bottom of the sea to extend horizontally. The amount of material *available* for the formation of a basis whereon the laterally growing corals may find footing is limited, and grows only in proportion to the circumference of a reef. The amount of material *required* for this purpose grows in a much more rapid proportion because it has to cover the surface of the growing cone.

Take a simple case. An ordinary straight fringing reef advances on a bottom of 10° inclination straight outward. At a distance of 280 metres from the shore a depth of 50 metres is reached. Further lateral growth is only possible on a talus of coral *débris*. 560 metres from the shore the depth is 100 metres. If the talus will lie at an angle of 45°, an amount of 50 cubic metres extended over a surface of 71 square metres will be necessary for the advance of each 1 metre's length of reef a distance of 1 metre. This talus is contributed from 50 square metres of growing coral (vertical reef face). At a distance of 5600 metres from the shore the depth will be 1000 metres. Every 50 square metres of growing reef face will have to furnish 950 cubic metres of material to enable the reef to advance 1 metre. These 950 cubic metres will be distributed over a surface of 1350 square metres. In other words, the reef will advance nineteen times as slowly as it does 560 metres from the shore, whilst the surface which is exposed to the dissolving effect of the sea-water has also increased nineteen-fold. Where an ocean-current strikes such an incline, no Globigerina ooze can be deposited on it, and here the dissolving action of sea-water will balance the accumulation of coral *débris* long before such a height—of 1000 metres—is attained. It is clear that as soon as such equilibrium is reached there is a limit to the further extension of the reef in that direction. On the opposite side, however, where ooze will accumulate and protect the advancing reef from solution, such advance would be possible, but on that side the growth of coral is notoriously slow. Certainly, when the foot of the reef has advanced to depths below the zone of protecting Globigerina ooze no further lateral growth in *any* direction will be possible, and on the whole I should not think that lateral growth can play any considerable part in the formation of great reefs. Only positive coast-line shifting has such a result. In places where there is no such coast-line shifting (Gulf of Suez) the reefs are exceedingly small and insignificant.

Although therefore lateral growth no doubt does take place, it is not the actual cause of the formation of the great coral reefs.

We must, I think, revert to Darwin's subsidence theory, which is equally proved by the untenability of the hypothesis suggested for the purpose of superseding it, and by the direct evidence of the structure of the Triassic reefs in the Eastern Alps, which have actually attained their immense thickness during a period of positive shifting of the coast-line. R. VON LENDENFELD.

Innsbruck.

Slugs and Thorns.

IN NATURE, vol. xli. p. 393, I pointed out that thorns might not always be a protection from snails and slugs, since they were found in the stomach of a European slug, *Parmacella*. In further confirmation of this view, I have to-day dissected a number of sharp, straight, reddish-brown thorns, over a millimetre long, from the intestinal tract of *Ariolimax niger*, var. nov. *maculatus*, a slug of rather doubtful affinities (possibly referable to *A. andersoni*), received from Dr. J. G. Cooper, who found it under drift-wood at Haywards, California. It is curious that the thorns do not pierce the intestine, but they appear to cause no inconvenience. T. D. A. COCKERELL.

West Cliff, Colorado, April 21.

COMETS OF SHORT PERIOD.

IT is now generally accepted that the periodic comets of our system did not originate in it, but are bodies captured from outer space by one of the planets, the parabolic orbits in which they approached the system being transformed into elliptical ones. On account of the perturbing action of Jupiter, however, the orbits of short-period comets are liable to considerable modifications, and it is practically impossible to identify two apparitions of the same comet without laborious computations of the perturbations which it must have been subjected to between the two epochs. But even such computations may lead to a negative result, for frequently comets quite distinct have elements very much alike, probably because they are parts of an old comet travelling along the same orbit at greater or less intervals.

In some recent investigations on the capture theory of comets (*Bulletin Astronomique*, June and July 1889), M. Tisserand developed a relation that might be employed to determine the possibility of identity of comets whose elliptical elements are known. This criterion depends upon the fact that the velocity of a body revolving round a central one is the same for equal radius-vectors. In the case of a comet having a parabolic orbit coming under the influence of a planet, the latter plays the part of the central body, and the relative velocity of the comet with reference to it will be the same at the point of entry into the sphere of attraction as at the point of departure from it, the one point being in the old orbit, the other in the new one. If two comets are identical, their velocities with reference to the perturbing planet will be the same at these points.[1]

M. L. Schulhof has discussed the possibility of identity of several comets by means of M. Tisserand's formula (*Bull. Astr.*, November and December 1889, and *Astr. Nach.*, 2964), and the following tables contain the values of n found for those having periods from 3·3 to 8·8 years. In the first table, the comets whose periods are well known are given; in the second, those having uncertain periods. Comets which have undergone strong perturbations since discovery, and those for which perturbations prior to the first known apparition have been calculated, are given more than once, and the year indicated for which the elements are found. The symbols used have their usual signification, and l is the longitude of the comet at the point of nearest approach to Jupiter.

Comets of Known Period.

Name of Comet.	Elements of Orbit.						
	n	π	☊	i	e	a	l
		°	°	°			°
1. Denning, V. 1881	0·414	19	66	7	0·83	4·23	223
2. Brorsen, 1842	0·466	112	104	46	0·76	2·99	284
,, 1890	0·475	116	101	29	0·81	3·10	284
3. Finlay, VII. 1886	0·483	8	52	3	0 72	3·54	205
4. Lexell, 1767	0·483	26	352	33	0·33	4·45	163
,, 1770	0·486	356	132	2	0·79	3·16	184
,, 1779	0·478	159	178	18	0·91	60·10	184
5. Biela, 1772	0·486	110	257	17	0·72	3·58	269
,, 1852	0·482	109	246	13	0·76	3·53	269
6. Wolf, 1868	0·492	6	208	29	0·39	4·18	211
,, III. 1884	0·497	19	206	25	0·56	3·58	210
7. D'Arrest, 1851	0·503	323	148	14	0·66	3·44	153
,, 1883	0·504	319	146	16	0·63	3·55	153
8. Faye, 1814	0·509	55	225	7	0·56	3·83	212
,, 1880	0·507	51	210	11	0·55	3·85	208
9. Winnecke, 1809	0·509	274	113	10	0·75	3·21	107
,, 1886	0·509	276	104	14	0·73	3·23	109
10. Tempel$_1$, 1869	0 527	43	297	5	0·66	3·11	223
11. Brooks, 1885	0·531	203	179	8	0·39	8·99	185
,, V. 1889	0·530	1	18	6	0·47	3·68	185
12. De Vico, 1678	0·542	323	163	3	0·63	3·07	143
,, I. 1844	0·537	343	64	3	0·62	3·10	163
13. Barnard, II. 1884	0·556	306	5	5	0·58	3·08	126
14. Tempel$_2$, 1873	0·562	306	121	13	0·55	3·00	126
15. Tempel$_1$, 1856	0·591	236	103	6	0 53	3·13	56
,, 1885	0·589	241	72	11	3·41	3·49	61
16. Encke, 1795	0·591	157	335	14	0·85	2·21	335

Comets of Uncertain Period.

1. Comet of 1585	0·484	10	38	5	0·70	3·61	213
2. Grischau, I. 1743	0·525	93	87	2	0·72	3·09	271
3. Helfenzrieda, II. 1766	0·493	251	74	8	0·86	2·93	80
4. Pigott, 1783	0·473	50	56	45	0·55	3·26	233
5. Blainpain, IV. 1819	0·517	68	78	9	0·71	3·11	248
6. Tuttle, III. 1858	0·527	201	175	20	0·67	3·52	357
7. Coggia, VII. 1873	0·484	86	249	26	0·76	3·19	255
8. Brooks, IV. 1886	0·553	230	53	13	0·61	3·41	54
9. Swift, VI. 1889	0·462	40	330	10	0·68	4·27	189

The value of n therefore found by the formula given is almost constant for the 21 known short-period comets, being contained within the limits 0·41 for Denning's comet, and 0·59 for Encke's and Tempel's comets.

It will also be seen that only five comets have their minimum distance to Jupiter's orbit between $l = 284^\circ$ and $l = 112^\circ$, while twelve have the point of nearest approach between $l = 153^\circ$ and $l = 233^\circ$. This unequal distribution along the ecliptic cannot be accidental, and

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[1] M. Tisserand expressed the criterion very approximately by the formula—

$$\frac{1}{a_1} - \frac{1}{a_2} = \frac{2\sqrt{A}}{R^2}\left(\cos i_2\sqrt{p_2} - \cos i_1\sqrt{p_1}\right),$$

where a_1, a_2, p_1, p_2, i_1, i_2, are the semi-major axes, parameters, and inclinations of the old and new orbits; A and R the planet's semi-major axis and radius-vector at the point of nearest approach. This relation may be divided up into two parts, having the form—

$$n = \frac{1}{a} + \frac{2\sqrt{A}}{R}\cos i\sqrt{p}.$$